

Dalton (J.C.)

MR. BERGH
AS A
COMMENTATOR.

A Criticism of Quotations.

FROM THE "NATION" OF OCTOBER 16TH, NOVEMBER
6TH AND NOVEMBER 20TH, 1879.

BY

J. C. DALTON, M. D.



NEW YORK:
TROW'S PRINTING AND BOOKBINDING COMPANY,
201-213 EAST 12TH STREET.
1880.



MR. BERGH AS A COMMENTATOR.

From The Nation, October 16, 1879.

IT seems that the discussion in regard to vivisection has once more broken out in the daily papers, with the usual exchanges of denunciation and defence. The present aspect of the controversy is essentially this: Mr. Bergh contends for a legal suppression of the practice; and, in order to influence the public mind in that direction, he portrays in strenuous language the iniquities of the physiologists, and denounces their results as uncertain and worthless. The doctors, on the other hand, say that his descriptions are exaggerated or untruthful, and his inferences false. It is proper that the unprofessional reader should have some means of judging between these antagonistic parties. If Mr. Bergh's statements are exact, let them have the weight which they deserve; if they are open to doubt, the public should be warned. An important element in this question would be that of faithfulness in the matter of *quotations*.

One of Mr. Bergh's favorite assertions is that the medical profession is opposed to vivisection; or, as he expresses it with a pleasing erudition, "From the time of Celsus, the Hippocrates of the Romans, till our own

day, when humanity is making itself heard, the best men have raised their voices against it." To establish this point he quotes from many well-known authors, and uses the weight of their names and language to discredit the propriety of experiments on animals. But in looking over these quotations we are struck by one peculiarity—that is, there is nothing to show where they came from. The value of a quotation, apart from the reputation of its author, depends upon its accuracy and the connection in which it is used. In the discussion of controverted topics it is usual to accompany a quotation with exact references, giving the title of the book from which it is drawn, and the page where it is to be found, in order that any one may refer to the original passage and satisfy himself that the citation is genuine. Mr. Bergh habitually omits this useful precaution. Most of his quotations give the name of the author, but no further reference which would enable the reader to verify their correctness.

Besides this, to the medical mind there is something a little odd in seeing such names as Colin, Carpenter, Longet, or Hunter arrayed as witnesses against the value of experimentation on animals. These men have passed their lives and gained their reputation in performing experiments or in collecting their results. That they should, nevertheless, believe and declare them to be worthless, has a puzzling look about it. It would be interesting to refer to the original language of these authors and find out what it means.

But in order to understand the matter fully, it is necessary to know, in the first place, from what source Mr. Bergh derives his scientific information.

In 1866 the London Society for the Prevention of

Cruelty to Animals published a prize essay on vivisection,* by Mr. G. Fleming, veterinary surgeon to the Third King's Own Hussars—an essay which is spoken of by a reviewer in the London *Athenæum* for September 22, 1866, as "ignorant, fallacious, and altogether unworthy of acceptance." After a perusal of the work we heartily agree with this estimate of its character.

It is from this essay that Mr. Bergh has drawn most of his ideas and much of his language. The similarity of expression, in many instances, is too close to admit of any other conclusion. Let us compare a few passages by way of example :

MR. BERGH, *in the N. Y. Evening Post, September 12, 1874.*

"From the time of Celsus, the Hippocrates of the Romans, till our own day, when humanity is making itself heard, the best men have raised their voices against it."



MR. BERGH, *in the N. Y. Evening Post, September 12, 1874.*

"The director of the imperial school of Alfort, in reviewing a long series of vivisections, felt himself compelled to ask," etc.

Fleming's Essay, 1866, page 25.

"From the time when Celsus, the Hippocrates of the Romans, blamed Herophilus of Chalcedon for having dissected the bodies of living criminals, which had been given over to him by the kings of Egypt, up to the present day, when the voice of humanity is making itself more loudly and forcibly felt on behalf of the lower animals, many and just good men have given their opinion as to its barbarity."

Fleming's Essay, 1866, page 40.

"The conscientious director of the imperial school at Alfort, in reviewing a long series of vivisections, has felt himself compelled to ask," etc., etc.

* 'Vivisection: is it necessary or justifiable?' London : Robert Hardwicke, 192 Piccadilly. 1866.

Mr. Bergh, in the N. Y. Evening Post, September 12, 1874.

"Is it not time that universal sentiment should put a stop to these horrid operations, which tend to harden the heart, extinguish those instincts which give man confidence in man, and make the physician more dreaded than disease itself?"

"It is time that universal resentment should arise against these horrid operations, which tend to harden the heart, extinguish those sensations which give man confidence in man, and make the physician more dreadful than the gout or stone."

There can be no doubt that Mr. Bergh has attentively read Fleming's essay, and is indebted to it for much of his inspiration.

One of the authors whom we have mentioned above as appearing in an unexpected light is Professor Colin, of the Veterinary School at Alfort, in France, and author of the 'Physiologie Comparée des Animaux Domestiques.' Mr. Bergh arrays this writer as one of his chief witnesses. In the New York *Herald* of February 13, 1867, he says:

"Colin, the author of a treatise on Physiology, wholly condemns vivisection."

In the New York *Tribune* of September 26, 1874, he enumerates Colin, with others, among those who consider experiments of this kind as "worthless," owing to unavoidable errors.

In the New York *Evening Post* of September 12, 1874, he cites the writer in question as follows:

"Monsieur Colin says: 'From the moment that a function is disturbed in its normal condition it changes its character, and all the others experience a like commotion and soon become suspended. The possibility of isolating physical or chemical phenomena is simply hopeless.'"

Now, what is the meaning conveyed by this language? The reader will, of course, infer that, in Colin's opinion, no possible result can be obtained from such experiments; and that, from the nature of the case, it is useless to attempt it. The fact is exactly the contrary.

The quotation is from a passage in Colin's 'Traité de Physiologie Comparée des Animaux Domestiques,' Paris, 1854, tome i. page 32. It occurs in paragraph iv. of the introduction, a paragraph expressly devoted to the subject, and entitled "De l'Expérimentation." The author is treating of experimentation in general, as a means of study in natural science, and the manner in which it should be employed in various cases in order to be useful. He is showing the difference which exists between experiments in chemistry and physics, which may be fully simplified, and those in physiology, which are more complex. As Mr. Bergh gives it, the quotation is incorrect. The genuine passage is as follows:

"Dès l'instant qu'une fonction est mise en dehors de ses conditions normales elle change de caractère, et dès qu'elle cesse de s'exécuter, toutes les autres (si elle est un peu importante) éprouvent des perturbations plus ou moins profondes, et bientôt se suspendent. La possibilité d'isoler les phénomènes physiques et chimiques, et l'impossibilité d'arriver à ce résultat en ce qui concerne ceux de l'ordre physiologique, établissent une différence capitale entre le mode d'expérimentation qui s'applique aux premiers et celui qui convient aux seconds. La différence est si grande que l'on ne voit pas ce qu'il peut y avoir de commun entre les deux modes; il est superflu pour nous de chercher à imiter des procédés inapplicables aux recherches physiologiques."

Colin, therefore, has not the remotest idea of condemning experimentation on animals in physiology. On the contrary, he is doing his best to show how it is to be made effective and trustworthy.

What excuse has Mr. Bergh to offer for this palpable distortion of his author's meaning? It is possible that he might refer us to his favorite Fleming's essay, and plead that he took his quotation at second hand, without consulting the original work. Such a defence, in any event, would be a lame one, but in this instance it would make the matter worse, because in Fleming's essay the translation is correctly given. Let us see how the two quotations look side by side.

MR BERGH, in the *N. Y. Evening Post*, September 12, 1874.

Quotation in Fleming's Essay,
page 26.

"From the moment that a function is disturbed in its normal condition it changes its character, and all the others experience a like commotion, and soon become suspended. The possibility of isolating physical or chemical phenomena is simply hopeless."

"From the moment that a function is disturbed or removed from its normal conditions it changes its character, and all the others (if it is a function of some importance) experience a commotion more or less profound and soon become suspended. The possibility of isolating physical or chemical phenomena, and the impossibility of isolating those which belong to the order of physiology, establishes a capital difference between the modes of experimentation as applied to the first and those which belong to the second. The difference is so great that it is impossible to see what there can be in common between the two modes. It is, then, superfluous for us to seek to apply a means of procedure which is inapplicable to physiological researches."

Besides the usual resemblance in phraseology between Mr. Bergh and Mr. Fleming, there is also a singular

difference between them in the sentence which introduces this quotation from Colin:

MR. BERGH, in the *N. Y. Evening Post*, September 12, 1874. From Fleming's *Essay*, 1866, page 26.

"I will quote from the work of a French physiologist, in this instance a staunch admirer of those men who have made themselves notorious for their practice of that branch of science. Monsieur Colin says," etc., etc.

"Let me quote from the work of French physiologist, *who is a great advocate for vivisection*, a stanch admirer of those men who have been notorious for their practice in that particular branch of science, and *who is himself a most enthusiastic experimenter*. M. Colin says," etc., etc.

Mr. Bergh is here endeavoring to prove that Colin is opposed to experiments on animals; and, in extracting his opening sentence from Fleming's essay, he has left out that part of it which shows that Colin in reality approved of them. The disloyal intent is too manifest to require comment. Our eminent philanthropist would have the "doors of the State prison" close upon such persons as do not meet with his approval. But if confinement in State prison were the legal penalty for tampering with an author's opinions and falsifying his language, I am afraid Mr. Bergh would have been there long ago.

From The Nation, November 6, 1879.

In the issue of October 16th I called attention to Mr. Bergh's misapplication of a passage in Professor Colin's 'Physiologie Comparée des Animaux Domestiques.' This citation is one of many which have been given to the public for the purpose of throwing discredit upon experiments on animals in physiology. The passage in question had been quoted as showing that its author distrusted experiments of this nature and disapproved of them. But a reference to Colin's original language made it evident, on the contrary, that he fully approved of them, and regarded them as valuable means of scientific research. Does not such a discrepancy need explanation? Mr. Bergh should not underrate the importance of this point. The quotations, as offered by him, are in the nature of evidence. They are used to show what have been the experience and opinions of certain eminent men in regard to vivisection. If this evidence is false, it makes a difference in the conclusions to be drawn from it. May we not hope that Mr. Bergh will explain in some way his misstatement with regard to Colin?

Besides Colin, there is another eminent physiologist, still better known to American readers, who is arrayed by Mr. Bergh as opposed to physiological experimentation on animals. This is Dr. William B. Carpenter, late Registrar to the University of London, whose books have long been familiar standards in physiology.

and natural history, in England and the United States. In the New York *Evening Post* of September 12, 1874, Mr. Bergh refers to him as follows :

“ Dr. Carpenter, than whom there is scarcely a higher authority, says : ‘ Take away an important and essential part of a living being, and it ceases to exist as such ; it no longer exhibits even a trace of those properties which it is our object to examine, and its elements remain subject only to the common laws of matter. ’ ”

This passage seems to have been taken, not from Dr. Carpenter’s original work, but from Fleming’s Prize Essay on Vivisection (London, 1866), which has been already shown, in the *Nation* of October 16th, to be the source from which Mr. Bergh has borrowed much of his controversial ammunition. The quotation appears in Fleming’s Essay, on page 50, accompanied by the introductory phrase: “ Dr. Carpenter, than whom we could scarcely have a higher authority on this subject.” etc., etc. Now, Dr. Carpenter’s connection with the famous Prize Essay is a curious one. He was one of those who served upon the Committee of Adjudication for the competing essays ; and, on examining Mr. Fleming’s production among the rest, he found it so full of misrepresentations that, as a member of the committee, he felt bound to declare his disapproval. Accordingly, the printed document, as published by the Society, contains an appendix, in which Dr. Carpenter corrects the false statements and inferences of the author, and repudiates the opinions attributed to himself. In the body of the essay references are made to the comments by Dr. Carpenter in the appendix ; so that whoever reads the first is at once referred to the second. Thus, the passage quoted above by Mr. Bergh, on page 50 of

the essay, refers by a foot-note to Dr. Carpenter's correction in the appendix, which is (page 75) as follows :

"I must entirely disclaim the general inference which the author bases on a limited proposition. I go as far as any one in the importance I attach to anatomical investigation, and in faith as to the value of the 'experiments prepared for us by Nature.' But the author ought not to ignore the explicit testimony I have borne to experiments in my chapter on the determination of the functions of the nerves."

Again, on page 23 of the essay there is the following passage by Fleming :

"The results achieved, however, looking at them from the most favorable point of view, be they valuable or otherwise, have cost an amount of suffering to sentient beings far beyond considerations of value or necessity, and which, when compared one with the other, fixes a perfectly just verdict of 'needless and cruel' against nine-tenths of the almost endless number of experiments performed by physiologists."

To this Dr. Carpenter replies in the appendix, page 73, as follows :

"I entirely dissent from this statement. If we knock out of the existing system of universally accepted physiological knowledge all that has been learned from experiment, and what experiment alone can reveal, we should go back to a depth of ignorance which must cause a most lamentable increase in human suffering, through the maltreatment of disease and injury which would be the result."

So Dr. Carpenter's opinion, like Colin's, is in reality favorable to experimentation. No one could have the slightest doubt about it who should read his original work ; and the above extracts show that the same knowledge is easily accessible to any one possessing a copy of Fleming's 'Essay.' But both Carpenter and Colin are paraded by Mr. Bergh as giving their testimony in its condemnation.

What shall we call this manipulation of the facts, used to convey an impression at variance with the reality? If we did not know that it came from a professional philanthropist we should be inclined to give it a very awkward name. It is plain that Mr. Bergh, in his eagerness to establish a particular point, has made use of means which are universally regarded as illegitimate. He has misapplied quotations, and even changed their phraseology, to represent opinions which are expressly repudiated by the authors themselves. Fortunately, a little expenditure of time and trouble will always be sufficient to detect a spurious issue of this kind and trace it back to its starting-point. Besides, it is somewhat of a relief to find that these scientific luminaries, occupying "the loftiest pedestals in the Temple of Fame," have not, after all, been stultifying themselves so much as we feared.

From the Nation, November 20, 1879.

In my previous enquiries into the curious change of front sometimes shown by quotations in the hands of Mr. Bergh, our attention was occupied by the cases of Colin and Carpenter. Both these gentlemen are foreigners, one of them being a Frenchman, the other an Englishman. But there is another eminent physiologist, who, though French by birth, has spent so much time in this country that he is not only widely known to us by reputation, but is personally familiar to many in the United States. This is Dr. C. E. Brown-Séquard, at present occupying the Chair of Medicine in the College of France as the successor of Claude Bernard. Dr. Brown-Séquard's studies have been mainly devoted to the nervous system, and he has sought, with great skill and untiring industry, to unravel its secrets by the experimental method.

It is unnecessary to say that experimental investigations in the nervous system require the greatest care in their performance and application. The structure of the parts is so complicated, and the possibility of their sympathetic reaction so constant, that every precaution is necessary to avoid uncertainty or failure. Nevertheless, success in this department, though difficult, is not impossible; and, with everything that still remains in doubt, a certain number of important facts with regard to the nervous functions have been established to the satisfaction of all. Dr. Brown-Séquard has won his

reputation, in great measure, by investigations on the spinal cord—a part which is at once of fundamental importance in the nervous system, and also less complex in structure than the brain and its appendages.

According to Mr. Bergh, Brown-Séquard has an exceedingly low opinion of experiments on the spinal cord; and he quotes him, in the New York *Tribune* of September 26, 1874, as expressing this opinion in very plain terms. The passage is as follows :

“Even Dr. Brown-Séquard himself, the very prince of experimenters, says: ‘I must say that it is impossible to know, while we make a section of parts of a spinal cord, what is the precise depth of the injury ; it is mere guesswork.’ ”

Evidently, then, there can be no use in making the section. As the whole object of such an experiment is to learn what nervous endowments belong to a particular part, if we do not know what part has been reached by the section we can get from it no useful information. But why did Brown-Séquard go on making such experiments when he knew that it was impossible to learn anything from them ? Perhaps he can tell us.

The above quotation is from Dr. Brown-Séquard’s ‘Lectures on the Physiology and Pathology of the Central Nervous System.’ (Philadelphia : 1860.) It is to be found on page 42, in a foot-note. Mr. Bergh gives the quotation correctly, so far as it goes. The trouble is, he does not give the whole of it. The entire passage is as follows :

“I must say that it is absolutely impossible to know, *while* we make a section of parts of the spinal cord, what is the precise depth of the injury ; it is mere guesswork. But if we study well the phenomena, and then, after having killed the animal, if we put the spinal cord in

alcohol, we render it hard, and we can ascertain exactly what is the extent of the incision. This is the means that I always employ in my experiments, and it is also the means employed by the committee appointed by the Société de Biologie in 1855 for the investigation of my researches on the spinal cord."

So it appears that Dr. Brown-Séquard has no doubt about the exact spot reached by his incision in the spinal cord ; and no one who has the opportunity of reading his original passage can have any doubt about it either. Would it be too much to say that the audacity of curtailing his language to give it an opposite sense has something about it almost ludicrous ?

I will touch upon an additional instance of this sort, more on account of the great name involved in it than because it is very different from the rest. In the New York *Herald* of September 30, 1879, Mr. Bergh denies that Harvey was the first discoverer of the circulation of the blood, and maintains that it is only a popular error to think so. But in any event, he assures us, it was not by the aid of vivisection that Harvey accomplished what he did ; and, to make the matter sure, he quotes the great author himself. Mr. Bergh says :

"While correcting this idea of Harvey's discovery, my principal purpose is to show that it was not to vivisection that he owes his fame, for nowhere, so far as I can learn, does he assert that he discovered the circulation by vivisection. On the contrary, he tells us : 'When I first gave my mind to vivisections as a means of discovering the motions and uses of the heart, and sought to discover these from actual inspection, I found the task truly arduous—so full of difficulties that I was almost tempted to think that the motion of the heart was only to be comprehended by God. My mind was greatly unsettled, nor did I know what I should conclude'; and he adds : 'It was reflection which led to the discovery.'"

Now, if any one can be presumed to know by what means Harvey reached his discoveries, such as they were, it is probably Harvey himself; and if he tells us, as implied above, that vivisections only left his mind "greatly unsettled" on this point, so much the worse for vivisections. But Mr. Bergh stops in his quotation at a very convenient place. If he had continued to the bottom of the page the effect would have been different. The passage is from the 'Works of William Harvey, M.D.' (Sydenham edition, London, 1847), at the commencement of chapter i., page 19. The succeeding paragraph is as follows:

"At length, and by using greater and daily diligence, having frequent recourse to vivisections, employing a variety of animals for the purpose, and collating numerous observations, I thought that I had attained to the truth, that I should extricate myself and escape from this labyrinth, and that I had discovered, what I so much desired, both the motion and the use of the heart and arteries; since which time I have not hesitated to expose my views upon these subjects, not only in private to my friends, but also in public in my anatomical lectures, after the manner of the Academy of old."

There is no such additional remark ("it was reflection which led to the discovery") anywhere in this chapter.

I think we must allow, therefore, that Harvey does attribute his discovery to the results of experiment, notwithstanding Mr. Bergh's difficulty in finding the assertion.





